DRY "MOUNTS" FOR THE MICROSCOPE.

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What shall we use to preserve dry mounts effectually? Many may think that nothing is easier; a cell of Brunswick black; a wax ring, or one of balsam; but the question is not thus easily to be disposed of. The writer has, within the last five years, mounted, or has had mounted under his supervision, some 15,000 slides of various microscopical objects, chiefly, however, foraminifera and diatoms; half of these were dry mounts.

Two things are important—the cell should be quickly and easily made, and the object when mounted in it should remain unchanged. There are very few cells as now made which will fulfil both these conditions, especially the latter. The deterioration of delicate dry mounts, and especially of test objects, sometimes within a few months after their preparation, but more or less certain in nearly every case, is well enough known.

All of the dry mounts of the Eulenstein series of diatoms, *e. g.*, which I have seen, are spoiled; and my cabinet is full of such preparations. Even Moller's do not escape, though they are, upon the whole, the most durable. I have abundance of amateur works that no doubt looked very beautiful just as they issued from the hands of the enthusiastic preparers, which are now, alas, mere wrecks; and worse than this, many choice and rare specimens, which I cannot replace, hopelessly ruined.

I believe that I was the first one to suggest the use of sheet wax for the bottoms of cells for foraminifera and other opaque objects, and of wax rings for diatoms and other transparent objects. (See Journal Quekett Club.)

The number of spoiled specimens, especially of diatoms and delicate transparent objects which I can now show, proves that this method of mounting is decidedly bad. I have lived to see the day when I shall be quite glad if the responsibility of suggesting such a nuisance as the wax ring can be transferred to some one else. For large opaque objects like most of the foraminifera, seeds, pollens, &c., the object itself is not so much injured, but the covering glass will, sooner or later, become covered (inside the cell), on the under surface, with a dew like deposit, which, when illuminated, will glisten almost like so many minute points of quicksilver, and though out of focus when the object is viewed, will show very disagreeably, like a thin gauze between; and with transparent objects these minute globules will not only dot the entire field, as so many dark or light points, but the object itself will appear as though it had been wetted.

Not long ago a well-known optician showed to me a spoiled slide "podura." The scales were very good and large—in fact, it was a slide which I had given to him, and it had been selected by myself in Beck's establishment in London as unexceptionably fine. This slide began slowly to show symptoms of "sweating." One scale after another appeared as though moisture had, in some mysterious way, penetrated to the objects; it was not water, however, for when the cover, after much trouble, had been removed, and warmed sufficiently to evaporate anything like water, the scales still exhibited the same appearance, and, in fact, the heat required to get rid of this apparent moisture was so great, that the scales were charred. When wax rings are used, this apparent wetting or "sweating" occurs quickly, and more disagreeable than this, innumerable elongated specks, possibly crystalline, appear all over the under surface of the cover-glass. The same trouble occurs when any of the ordinary asphalt preparations are used, and the only cement which I have thus far found to be tolerably successful is shell-lac thoroughly incorporated with the finest carbon (diamond black) such as is used in the preparation of the best printing inks; the solvent being alcohol, these rings dry rapidly, and the cover is attached by heating. Even these rings cannot be trusted, unless thoroughly dry, and spontaneous drying is better than baking. I have had preparations spoiled after mounting on asphalt rings, which had been made for over a year, and which had been subjected for several hours to the heat of a steam bath. With large, somewhat course objects, the defect is not so marked, but with delicate ones, and especially test objects, it is simply a nuisance. With care I think the shell-lac rings may answer pretty well. I have not tried the aneline colored rings. The moisture (whatever it is), and the crystalline specks, appear to be derived from the vaporizable parts of the wax, or cement, given off under conditions where one would suppose such a thing impossible; it is however a fact; I have the proof of it, and I dare say hundreds of others have, too plainly evident.

There is another mode of making cells which promises well for permanence. My attention was first called to this method by Dr. Tulk, of London, who suggested for this purpose the thin gutta-percha tissue, used by surgeons in the place of oiled silk, I have had special punches made, which cut neat rings from this tissue, and I have used these rings with the greatest satisfaction. I have no preparations of my own more than about two years old; these so far, show no signs of change. Dr. Tulk informs me that he has them ten years old, and still good as when new. I have noticed that in some recent papers in the mineralogical journals the writers, who with little experience, have so lauded wax rings, speak of "thin rubber" for rings, evidently they have seen somewhere the gutta-percha mount, and supposed it rubber—the latter will not answer, melted rubber will not become hard. One beauty of the gutta percha ring is the very moderate heat required; it is thus available for many objects which might be injured by the greater heat necessary for the asphalt or shellac rings. As these rings in the arrangement which I have spoken of, can be rapidly made, and as they can be kept for any length of time (shut away from the dust), they are at any moment ready as well as convenient for use. The preparation is first arranged, dried or burnt on the cover, the slide cleaned, a ring laid on the centre and on this the cover is placed; the whole is now

held together by the forceps, and *slightly* warmed, just sufficient to soften the gutta percha; the forceps may now be laid aside, or used simply to press the cover home, warming the slide gently, also the cover; the perfect contact of the softened "tissue" with the cover and slide is easily recognized, and with a little care this can be effected very quickly, and nothing further is necessary. A finishing ring of colored cement makes a very neat mount, but it is not necessary.

ON MULTIPLE SPECTRA.

"Nunc age, quo motu genitalia materiai
Corpora res varias gignant, genitasque resolvant
Et qua vi facere id cogantur."

Lucretius ii., 61–2.

"Prima moventur enim per se primordia rerum:
Inde ea, quæ parvo sunt corpora conciliatu,
Et quasi proxima sunt ad vireis principiorum,
Ictibus illorum cæcis impulsa cientur
Ipsaque, quæ porro paulo maiora, lacessunt."

Lucretius, ii., 132–6.

"It is conceivable that the various kinds of matters, now recognized in different elementary substances, may possess one and the same ultimate or atomic molecule existing in different conditions of movement.

"The essential unity of matter is an hypothesis in harmony with the equal action of gravity upon all bodies."—*Graham's Researches*, p. 299.

In a recent paper* I showed that a study of the minute anatomy of spectra, both terrestrial and celestial, forces upon us the conclusion that both in the electric arc and in the hottest region of the sun the so-called chemical elements behave after the manner of compound bodies.

I then dealt more especially with the question of the basic lines in the various spectra, and it is clear that if at any one temperature, there be some lines only truly basic in the spectrum of any element, we at once divide the lines visible at that temperature into two groups, those which are basic and those which are not. This would give a compound origin to the lines, and this is the real point.

It is now years ago since the view was first held that the elementary bodies had double spectra, that is, that each, of at all events several, under changed conditions of temperature or electric tension, gave us now a fluted spectrum and now one composed of lines.

I glimpsed the idea some time afterward that the line spectrum was in its turn in all probability a complex whole, in other words that it was the summation of the spectra of various molecular groupings.

Recent work has to my mind not only shown that this is true, but that in the case of many bodies the complexity, and therefore the number, of the molecular groupings which give rise to that compound whole called a line spectrum, is considerable.

It is therefore important from my point of view to reconsider the evidence on which the assertion that the fluted bands and the line spectrum (taken as a whole) of a substance really belong to that substance, because if we find that this must be accepted and that it can easily be explained on the view that the two kinds of spectra are produced by different molecular groupings, the fact of other molecular groupings, giving rise to a complex line spectrum can be more readily accepted, contrary though it be to modern "chemical philosophy," as taught at all events in the text-books.

Plücker and Hittorf were, I believe, the first to point out that the same chemical substance, when in a state of gas or vapor, gave out different spectra under different conditions. On this point they wrote fifteen years ago:

"The first fact which we discovered in operating with our tubes . . was the following one:

"*There is a certain number of elementary substances which, when differently heated, furnish two kinds of spectra of quite a different character, not having any line or any band in common.*

"The fact is important, as well with regard to theoretical conceptions as to practical applications—the more so as the passage from one kind of spectrum to the other is by no means a continuous one, but takes place abruply. By regulating the temperature you may repeat the two spectra in any succession *ad libitum*." (Plücker and Hittorf on the Spectra of Ignited Gases and Vapors: *Phil. Trans.* Royal Society, 1865, part i. p. 6.)

Angström, whose name must ever be mentioned with the highest respect by any worker in spectrum analysis, was distinctly opposed to this view, and in the text which accompanies his *Spectre Normal* we find the following statement:

"Dans un Mémoire sur les spectres 'doubles' des corps élémentaires que nous publierons prochaînement, M. Thalén et moi, dans les Actes de la Société des Sciences d'Upsal, nous traiterons d'une manière suffisamment complète les questions importantes qu'on peut se proposer sur cet intéressant sujet. Pour le présent, je me borne à dire que res résultats auxquels nous sommes arrivés, ne confirment aucunement l'opinion émise par Plücker, qu'un corps élémentaire pourrait donner, suivant sa température plus ou moins élevée, des spectres tout-à-fait différents. C'est le contraire qui est exact. En effet en augmentant successivement la température, on trouve que les raies varient en intensité d'une manière trèscompliquée, et que, par suite, de nouvelles raies peuvent même se présenter, si la température s'élève suffisamment. Mais, independamment de toutes ces mutations, le spectre d'un certain corps conservera toujours son caractère individuel." *

Angström did not object merely on theoretical grounds. He saw, or thought he saw, room to ascribe all these fluted spectra to impurities.

He was strengthened in this view by observing how, in the case of the spectra of known compounds, there were always flutings in one part of the spectrum or another; a rapid induction naturally, therefore, ascribed all flutings to compounds. The continuity of the gaseous and liquid states of matter, let alone the continuity of Nature's processes generally, never entered into the question. For Angström, as for the modern chemist, there was no such thing as evolution, no possibility of a close physical relationship between elements, so called, driven to incandescence from the solid state, and binary compounds of those elements.

In a memoir, however, which appeared after Angström's death, and which, though under a different title, was in all probability the one referred to, this opinion was to a large extent recalled, and in favor of Plücker's view, in the following words:—

". . . Nous ne nions certainement pas qu'un corps simple ne puisse dans certains cas donner différents spectres. Citons, par exemple, le spectre d'absorption d'iode que ne ressemble en aucune facon au système des raies brillantes du même corps, obtenues au moyen de l'électricité; et remarquons de plus qu'en général tout corps simple, presentant la propriété d'allotropie, doit donner à l'état d'incandescence des spectres différents, pourvu que la dîte propriété de la substance subsiste non seulement à l'état gazeux du corps, mais encore à la température même de l'incandescence. . . .

"Le soufre solid possède, somme on sait, plusieurs états allotropiques, et, d'après certaines observations, ce corps, même à son état gazeux, prendrait des formes différentes. Par conséquent, en supposant que cela soit vrai, le soufre gazeux doit donner plusieurs spectres d'absorptio, tandis que la possibilité d'un seul on de plusieurs spectres brillants dépendra de la circonstance suivante, savoir si les états allotropiques plus complexes de cette substance supporteront la température de l'incandescence, avant de se décomposer.

*"On the Necessity for a New Departure in Spectrum Analysis" (NATURE, vol. xxi. p. 8.)

* Angström sur "Le Spectre normal du Soleil," page 39.